VOLUME XXVII NUMBER 1 WHOLE NUMBER 220

THE JOURNAL

OF

THE ROYAL ASTRONOMICAL SOCIETY OF CANADA

DEVOTED TO THE ADVANCEMENT OF ASTRONOMY AND ALLIED SCIENCES

EDITOR
C. A. CHANT
University of Toronto, Toronto

ASSOCIATE EDITORS

J. S. PLASKETT
Director of the Dominion Astrophysical Observatory, Victoria, B.C.

J. PATTERSON
Director of the Meteorological Service of Canada, Toronto

R. MELDRUM STEWART
Director of the Dominion Observatory, Ottawa

PUBLISHED MONTHLY
(Ten numbers per year)

January, 1933

PRINTED FOR THE SOCIETY
TORONTO: 198 COLLEGE ST.

THE JOURNAL

OF

THE ROYAL ASTRONOMICAL SOCIETY OF CANADA

VOL. XXVII, No. 1. JANUARY, 1933 WHOLE NO. 220

Articles PAGE

The Foundations of Earth-structure Theory - *E. A. Hodgson* 1
Ludendorff's Studies in the Astronomy of the Mayas *Roy K. Marshall* 11
The Constitution of the Stars (Radio Talk) - *R. K. Young* 27
Preliminary Report on Meteorological Conditions at Montreal and Louiseville during Afternoon of August 31, 1932 *Andrew Thomson* 32
Notes on the Variations of Temperature, Humidity, and Wind during the Eclipse of the Sun at Magog, August 31, 1932; with Comments on the Risk of Dew, and the Use of Temperature Observations - - - - *A. Norman Shaw* 34

Review of Publications

Das lokale Sternsystem, by F. Becker; *Die Rotation der Milchstrasse*, by H. F. Bottlinger (*R.K.Y.*) - - - - - 39
Change in the Reduction of the Occultations Observed in 1933 - *E. W. Brown and D. Brouwer* 40

News and Comments

Notes from the Dominion Astrophysical Observatory - *W.E.H.* 41

Notes and Queries

A Hundred Years Ago—Peculiar Effects on Eclipse Photographs Progress in Astronomy in 1932—Old Astronomical Records in Japan—Was this an Auroral Display?—Death of Weston Weatherbee - - - - - - - *C.A.C.* 42

Meetings of the Society—

At Winnipeg—At Vancouver - - - - - - - 47

Upon request, made previous to publication of article, contributors will be supplied free either with fifty copies of the issue containing the article, or with the sheets containing the article attached to a printed cover. If separate reprints are desired the cost will be as follows:

	4 pp.	8 pp.	12 pp.	16 pp.	20 pp.	24 pp.
50 copies	$3 50	$5 50	$ 9 50	$10 00	$13 00	$16 00
100 copies	4 00	7 00	10 00	11 00	14 00	18 00
200 copies	4 50	8 00	11 00	12 00	15 50	20 00

Covers, extra, 50 copies, $1.50; 100 copies, $2.00; 200 copies, $2.50.

Business correspondence, remittances, etc., should be addresed to J. H. Horning, Treasurer of the Society, 198 College Street, Toronto.

Communications regarding Library matters should be addressed to the Librarian.

$2.00 per annum. Single numbers, 25 cents.

Subscription to the JOURNAL is included in membership fee.

FOR SALE—5-inch refractor by Ross of London; first quality; equatorially mounted with clock and circles; 6 eyepieces, sun-glass, etc. Price, $475. HAROLD BROWN, 2108 Danforth Ave., Toronto.

THE JOURNAL
OF
THE ROYAL ASTRONOMICAL SOCIETY
OF CANADA

Vol. XXVII, No. 1 JANUARY, 1933 Whole No. 220

THE FOUNDATIONS OF EARTH-STRUCTURE THEORY

By Ernest A. Hodgson

IT is significant that man knew, in considerable detail, the structure of the solar system, wide spread about him, several centuries before he really knew anything of the structure of the earth beneath his very feet. His knowledge of the former was developed through data made available to him by light rays. His knowledge of the latter, in so far as he has as yet attained it, is based on data made available to him by seismic rays. The study of the celestial structure may be carried out by individuals in single localities. The emission of the light energy is continuous and observations may be made at known times, being subjected otherwise only to meteorological limitations. The seismic energy is emitted at times and from foci which cannot be predicted, nay, which must be determined after the event in every case if the data of the earthquake concerned are to be useful. This requires that the attack on the problem of geostructure be a collaboration of effort; recording must be continuous, absolute time must be determinable for each recording station to within a second or less. The wonder is not that we have been so long acquiring what knowledge we have of the earth's internal structure, but that we have been able to arrange and to maintain the international seismographic collaboration required.

The foundations of any satisfactory geognostic theory must needs be laid by seismology. A particularly close co-operative effort on the part of workers in all the related sciences is, however,

requisite for the rearing of its detailed superstructure. It often happens that the enthusiastic architect of such a theory pays too scant attention to the nature of the foundations on which he builds and fails to appreciate the relative strength of their various parts. We propose to consider, as fully as may be done within the limited compass of this paper, the restrictions inescapably placed on the development of an acceptable theory by the basic deductions from seismographic data.

The evidence, which we shall examine more in detail presently, leads us to deduce that the earth is built up as a series of concentric spherical shells. Let us reduce the problem to its simplest terms. At a depth which, for present purposes, may be set down as forty kilometres, more or less, lies what we shall here call the upper discontinuity, though it is recognized that others less pronounced or at any rate less generally distributed about the earth, are found to exist above it in most of the few locations where a seismic study of this upper crustal structure has been made possible by a concentration of well-equipped seismographic stations within a radius of five hundred kilometres or so of the epicentre of a sharply defined earthquake of not too great intensity.

For the purposes of this discussion we may disregard any discontinuity other than the upper one mentioned above and another at a depth of 2900 km. which we shall call simply the lower discontinuity. Let us think then of an earth made up of an irregularly layered crust lying above the upper spherical surface of discontinuity, beneath which to a depth of approximately 2900 km. we have a thick spherical shell—the mantle. The lower boundary of the mantle is our so-called lower discontinuity. Below this we come to a sphere of radius approximately 3760 km.—the core. Methods of studying conditions in the crust may here be termed crustal methods, those applicable to the intermediate shell mantle methods, and those peculiar to the inner sphere core methods.

The layer or layers lying above the upper discontinuity have been studied in detail only in three general regions, Europe, Japan, and California. Such studies can be carried out only where there is a concentration of well-equipped observing stations in a fairly-active seismic region. Different researches have deduced depths

to the upper discontinuity as we have defined it, which lie within the range fifteen kilometres to sixty kilometres. Crustal methods for studying geostructure are most interesting, but their discussion, if adequate, would be too lengthy to be included in this paper. We shall devote our attention to the structure of the mantle and the core, considering crustal methods only so far as they affect the application of the other procedure.

At the time of an earthquake, energy is released from a focus which for our present purposes may be considered a point. This focus lies at a depth which is usually found to be less than thirty kilometres, but which is in some cases of the order of 500 km. This energy is propagated in the direction of diverging seismic rays by two types of elastic waves—compressional and shear. The velocities of propagation differ, the former being always the greater. Seismologists have adopted the term *P*-wave to indicate the preliminary or compressional wave, arriving first at an observing station. They apply the term *S*-wave to the secondary or shear wave. It is termed secondary because it arrives after the *P*-wave, but it is also of secondary value in determining earth structure, since it is registered while the seismograph is still disturbed by the earlier phases and so may not be determined so precisely either as regards time of arrival, amplitude, or plane of vibration. The two velocities are frequently denoted by V_P and V_S respectively, being evaluated by

$$V_P = \sqrt{\frac{\lambda+2\mu}{\rho}} \text{ and } V_S = \sqrt{\frac{\mu}{\rho}},$$

where ρ denotes the density and λ and μ are the so-called Lamé constants. The identification of these two phases as read on the seismograms with the two types of elastic waves appears to be established beyond a peradventure.

A shear wave may not be propagated by a fluid. For various reasons it is thought that the earth's core may be fluid. It becomes of interest to determine whether the *S*-wave penetrates the core or not. It has not so far been positively identified to the general satisfaction of those studying the problem. Further work is still being done along this line.

Let us now note a fact which is of fundamental importance.

The seismological data are reduced on the basis of various assumptions which we shall discuss presently and which may or may not be true. But, even if all were rigorously established, these data could yield, for the deep-lying mantle, only the velocity of the wave or waves at various depths. Since the arrival of the P-phases can be determined with greater precision than can the S-phases, the law of variation of compressional wave velocity with depth is likely to be of a higher order of precision than any directly determined law of variation of shear wave velocity with depth. We may express V_P and V_S in somewhat different form, as follows:

$$V_P = \sqrt{\frac{E(1-\sigma)}{(1+\sigma)(1-2\sigma)\rho}} \text{ and } V_S = \sqrt{\frac{E}{2(1+\sigma)\rho}},$$

where σ and E denote respectively two other elastic constants, namely, Poisson's Ratio and Young's Modulus. If we suppose V_P and V_S to be determined with precision from observational data, we have but two equations to evaluate three unknowns. We may not say with certainty, for example, what is the law of variation of density with depth. We must make assumptions with regard to one of these quantities in order to evaluate the other two. Poisson's ratio has the value 1/4 to 1/3 for most surface conditions. If we assume its value at any given depth and have values for V_P and V_S based on observational data, we may deduce values for the density for Young's modulus. But, it is evident that these deduced values will be no stronger than our assumption. The values for the shear wave velocities may be obtained from the deduced values for the compressional wave multiplied by a constant obtained by assuming a value for Poisson's ratio. For example, if that ratio be taken as 1/4, the equations last given above reduce to a form yielding the ratio:

$$V_P/V_S = \sqrt{3}.$$

We see then that the seismological data yield only values for velocity at depth and that the compressional wave velocities are determined with the greater precision. These are based on the time-distance curve for the compressional wave—the P-curve. To make such a curve of any value, the earthquake on which it is

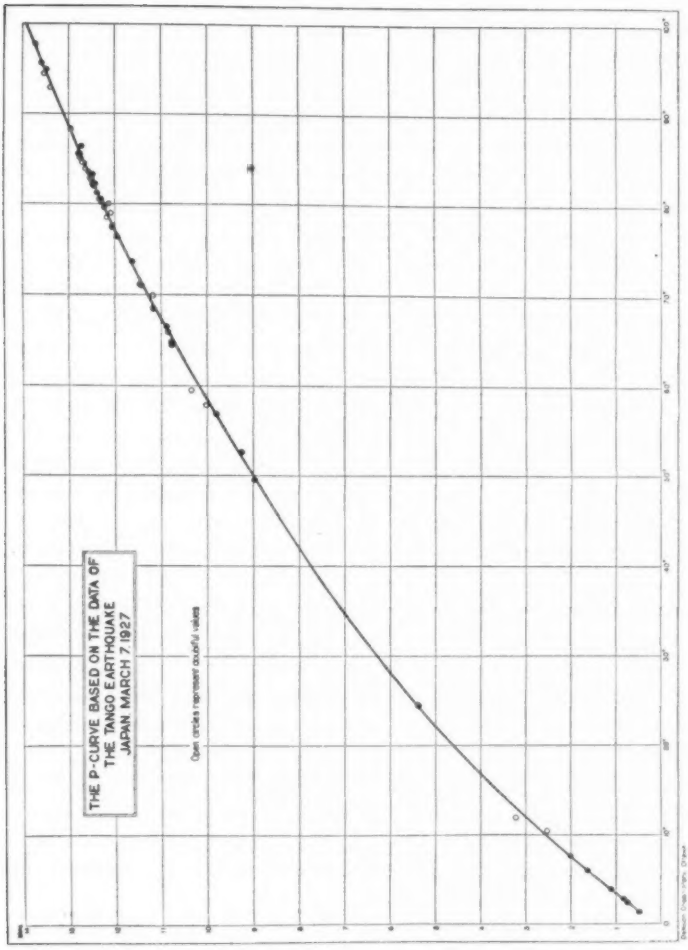
THE P-CURVE BASED ON THE DATA OF
THE TANGO EARTHQUAKE
JAPAN MARCH 7 1927
Open circles represent doubtful values

Fig. 1

based must be very sharply defined, the position of the focus, including its depth must be determinable, as must also the time of origin. As we have seen, there are but three regions in the world to-day where an earthquake may take place and such initial conditions for a *P*-curve be measured precisely. A further difficulty arises. In Fig. 1 we have a map showing the azimuth and distance values for the ninety-odd stations whose records were made available to the writer for a study of the Japanese earthquake of March 7, 1927. It is apparent that there is a lack of stations for distance within the ranges 20° to 60° and 100° to 180°. The resulting *P*-curve is given in Fig. 2. The same lack appears. There is not a point on the surface of the earth where we might expect to locate the origin time and the focus of an earthquake with precision and also have a distribution of stations throughout the entire distance range. Data from several earthquakes must be combined. For any focal region, however, it will be difficult to fill in with precision which shall be fully satisfactory the distance range 20° to 50°. Seismographic stations, well-equipped, are still urgently required in certain strategic positions if we are to make a concerted effort to lift the determination of velocities within the earth to a higher plane of precision.

Mantle methods fall into two classes, indirect and direct. An indirect method involves the assumption of a law of variation of velocity with depth. Such methods are far from satisfactory and the results obtained may be dismissed as of an order of accuracy lower than that obtained by the direct method. The latter, usually called the Herglotz-Wiechert method, is exceedingly laborious, involving many mechanical measurements of tangents to a satisfactorily accurate *P*-curve and the mechanical integration of a second, tediously-deduced curve. The entire work must be repeated for each point on the desired velocity-depth curve. To be of value, meticulous care must be taken in each step of the mechanical procedure.

Some of the assumptions on which the method rests are:

1. The focus of the earthquake lies in the surface.
2. No discontinuities are traversed by the rays which reach the surface for each distance range concerned.

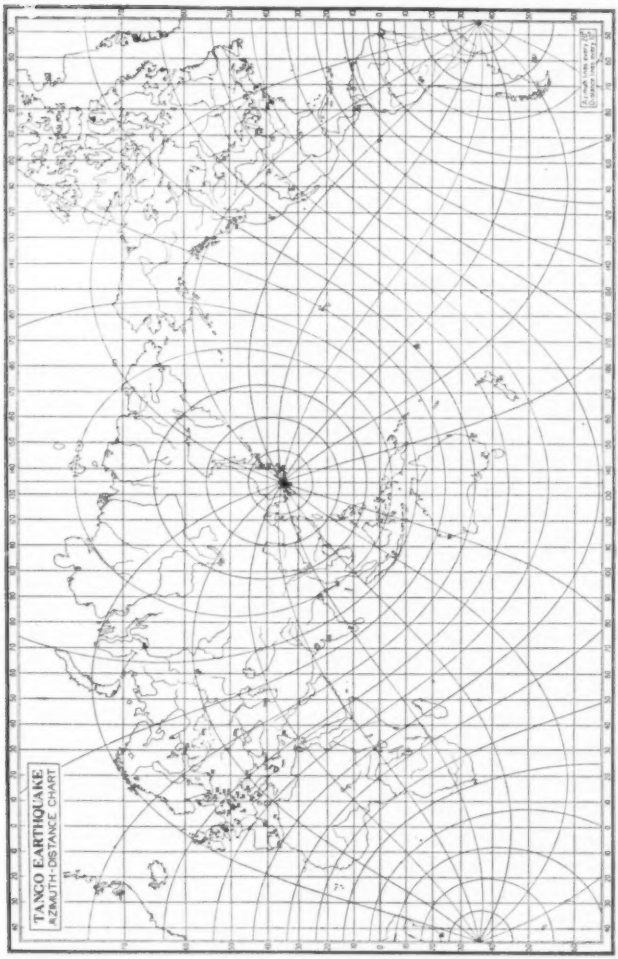

Fig. 2

3. The velocity increases continuously with the depth, the velocity-depth function being also differentiable throughout.
4. The laws of reflection and refraction (Snell's law) applicable to light rays apply to the seismic rays within the earth.
5. Each seismic ray is symmetrical about the radius through its vertex, the earth being considered a perfect sphere.
6. The true velocity (as differentiated from the apparent velocity given by the *P*-curve) for the surface horizon is constant for the entire earth. It is assumed to be known together with a *P*-curve giving time-distance values for an earthquake whose focal position and origin time are known.

Realizing that the first two assumptions are certainly not tenable, it might be inferred that the method may not be applied. At the time it was developed, surficial discontinuities were not known to exist and the focus of an earthquake was believed to lie so near the surface that no appreciable error would be introduced by ignoring the possible slight depths involved. With regard to assumption (6), we may say that the surface horizon velocities are not everywhere constant and may not be assumed to be so. However, in applying the method, Wiechert and Geiger, for example, assumed a constant true velocity at the surface which was very greatly in error, being about fifty per cent. too great. They assumed that $V^P = 7.174$ km/sec., whereas the average value is now known to be 5 km/sec. or less. This erroneous assumption was fortunate, as we shall see, and renders their results almost as free from error as regards this point as they would be if based on the best available modern data.

Surface methods determining the number of layers above the upper discontinuity, as we have called it, have been applied only in a few favourable regions, as we previously noted. But, wherever they have been applied, a true velocity of 7.75 to 8.0 km/sec. has been found for the compressional wave below that discontinuity. If now we could apply a correction to each time and each distance value in the *P*-curve, we might determine for each observing station the time required to traverse the arcual distance *AB* of Fig. 3 and we could build up a *P*-curve for an hypothetical earth stripped to the upper discontinuity. To do this accurately we should require a knowledge of surficial structure at the focus and at each observing

station. Such complete data cannot be hoped for at present, but one may work with an earthquake or earthquakes for which focal conditions are known, enabling him to determine the arcual distance correction from the epicentre at E to the point C and the time correction corresponding to the travel time from the focus at F to

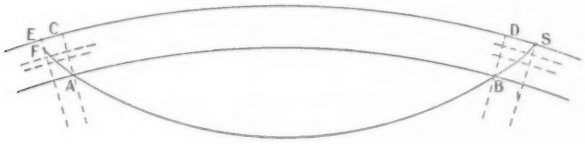

Fig. 3

the point A. He may then apply approximate corrections for each station, estimating the arcual distance DS and the time required to traverse the last part of the ray from B to the observing station at S. Applying these corrections to his measured distance ES and his determined time from focus to the station concerned, he obtains one point on his required P-curve for the stripped earth. The true velocity for the horizon immediately below the upper discontinuity

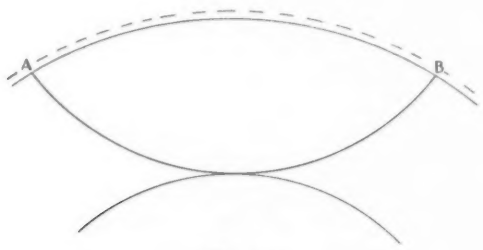

Fig. 4

being adopted as say 7.75 km/sec., observational data are reduced to conditions under which the assumptions are presumably tenable. We may now proceed to determine velocities at various depths down to the vertex of the ray which just tangents the lower discontinuity as shown in Fig. 4.

If the mantle velocities were accurately known we could, by a procedure not altogether unlike that used to strip off the surficial layers, strip the core and obtain a *P*-curve for it. Unfortunately it would not be possible to determine the true velocity of the compressional wave for the horizon immediately below the lower discontinuity, without making further assumptions. Improved studies of the core velocities cannot be made until we have re-determined the mantle velocities on the basis of the best available *P*-curve data.

Some of the earth-structure theory we now hold is established beyond a doubt. Other parts present anomalies which invite further investigation. The subject is an intensely interesting one which, however, requires long years of patient research on the part of many. It behooves us, step by step, to distinguish carefully between those details which rest on strong foundations and those which are but indifferently supported by deductions from observational data, made on the basis of assumptions which are but tentatively adopted.

Dominion Observatory,
Ottawa, Canada.
October 10, 1932.

LUDENDORFF'S STUDIES IN THE ASTRONOMY OF THE MAYAS

By Roy K. Marshall

Prefatory Note

DR. H. LUDENDORFF of the Astrophysikalisches Observatorium, Potsdam, has recently become interested in the interpretation of the numbers and dates found in the Mayan inscriptions and manuscripts, and has published, in the papers of the Preussische Akademie der Wissenschaften, his ideas concerning the astronomical and calendrical significance of these records. His five articles are to be found in *SB. der Akademie,* 1930. V, 1930. XVIII, 1931. I, 1931. III, and 1931. VIII. Through the gracious permission of Dr. Ludendorff and the Preussische Akademie der Wissenschaften I am able to present here a condensed statement of the content of these various articles, for those who do not have access to the originals. Reprints of the articles, in German, may be obtained from the Akademie for the sum of 8 RM, about $2.25. Any inconsistencies which may appear to a reader must be laid at my door, for I am sure that in Dr. Ludendorff's original German rendition none exist. It is conceivable that in the attempt to contract into a dozen or so pages what occupied about ninety pages of German some omissions may have been made which may be confusing to the reader. Insofar as it is possible, we shall adhere to the original outline of the discussion of the astronomy of the Mayas, as written by Ludendorff; it may be necessary in a few places to supplement a discussion with some material found later on in the series of papers. The section headings given here correspond to the titles of the five separate papers written by the German investigator. R.K.M.

On the Origin of the Tzolkin Period in the Calendar of the Mayas

The Mayas were the natives of that portion of Central America often referred to as the "thumb" of Yucatan, and the immediate

region to the south. The time of their emergence as a highly-developed people is rather uncertain, but we do know that the glorious period in their history was from 300 to 600 A.D., after which the forest engulfed their cities and the natives moved northward to the end of the peninsula of Yucatan. A second period of culture—a renaissance, as it were—occurred from about 1000 A.D. to 1400 A.D., when again the stone cities were in large measure abandoned to the jungle. The Spaniards arrived early in the sixteenth century, and as usual failed to gain the confidence of the natives, so that no definite knowledge of the culture of the Mayas was passed on to the white man. The usual "conversions" to Christianity were made, aided by the clever devices of the Inquisition, and all details of the religion, language, and learning of the Indians have had to be reconstructed from the stone ruins and three later-day copies of earlier manuscripts. The Mayas yet live there, but they know no more of the significance of the ruins than do the white men for whom they gather chiclet in the jungle. They are round-headed, having a cephalic index of 85.0 (obtained by multiplying by 100 the quotient of the breadth of the head by the length). They often wear light beards and moustaches, and their eyes are very dark brown and bear a strong resemblance to those of Mongolians, as far as their alignment is concerned. They are a short race, as a rule, averaging only slightly more than five feet in height. With this brief historical and racial outline we shall have to turn at once to the material of Dr. Ludendorff's papers. The interesting book by H. J. Spinden—*Ancient Civilizations of Mexico and Central America,* American Museum of Natural History, Handbook Series No. 3, Third and Revised Edition, New York, 1928—is heartily recommended for further reading.

The origin of the tzolkin period of 260 days which appears in the calendar of the Mayas (as well as in that of the Aztecs, where it bore the name "tonalamatl") has been a much discussed feature. None of the various conjectures has been accepted, and as late as 1928 Spinden said that this cycle of 260 days "corresponds to no natural time period and is an invention pure and simple". Ludendorff has suggested an interpretation of this unusual time period in terms of a submultiple of the eclipse year.

The complete scheme of Mayan chronology is divided into three parts, as follows:

(1) There was first a number which indicated how many days had elapsed since the beginning of the time count. It corresponded in all respects except that of origin-date with our familiar Julian Day.

(2) In the second place the position of the day within the 260-day tzolkin period was indicated.

(3) Third and last, the place of the day within the 365-day solar year of the Mayas, the haab, was given.

The day-numbering of the Mayas, hereafter referred to as the "long count", begins with a day in the dim and distant past, and each day bore a number which was greater by unity than that of the preceding day. The numbering system of the Mayas was vigesimal, although there is one violation of it. The following names were applied to the various units:

The unit of a day ..... 1 kin;
20 kins = 20 days ..... 1 uinal;
18 uinals = 360 days ..... 1 tun;
20 tuns = 7200 days ..... 1 katun;
20 katuns = 144000 days ..... 1 baktun.

The violation of the vigesimal system occurred at the tun, since this was not considered 20×20 days = 400 days, but only 18×20 days = 360 days, apparently because this is as close as possible to the solar year of 365 days. A certain Mayan date might thus be given in the long count:

9 baktuns, 12 katuns, 16 tuns, 0 uinals, 8 kins,

in which the ideas of the units were expressed by means of appropriate glyphs or symbols and the numbers expressed by combinations of bars and dots, a bar corresponding to five units and a dot a single unit. The manuscripts usually omitted the glyphs for the baktuns, tuns, etc., and the dates were written simply as, for example,

9 . 12 . 16 . 0 . 8,

except that the numbers were written vertically, with the index for the baktuns at the top. It is highly significant that the Mayas knew and used the zero, many centuries before the people of Europe

recognized a need for the character. To convert this date into a number in the decimal system would require multiplication of 144000 by 9, adding to that 7200 by 12, then 360 by 16, and finally 1 by 8, giving a total of 1388168, and this date actually occurred during the period of Mayan culture, so that we may be sure that the history of the people extended far into the past, in order to embrace that many days.

The tzolkin adhered in so far as possible to the vigesimal system, being composed of 13 periods of 20 days each. Twenty day names were introduced, corresponding with our Monday, Tuesday, etc., and with these twenty names the numbers 1 to 13 were combined in such a way that each day of the 260 bore a unique designation consisting of a number and a name. We may see how this was accomplished if we write down the letters from A to T in a vertical column, letting each letter represent a day name of the Mayas. Then, beginning at the top, write down the numbers from 1 to 13, each number opposite a day name. When the fourteenth letter is reached, write down 1 again, followed by 2, etc., until the bottom of the first column is reached, where the number 7 will appear opposite the letter T. Then begin again at the top, and put down the number 8 opposite A, in the second column of numbers, and again continue to 13, and follow it with another series from 1 to 13, and so on, until the number 1 again appears opposite the letter A, as the 261st day, which is really the first day of the next cycle of 260 days. A day then would be designated by the combination of the number opposite it and the letter (or day name) in the very first column; the ninety-seventh day, for example, would bear the designation 6 Q. The actual names of the days are known, and may be found in Spinden's book, mentioned above.

The solar year (the *haab*) contained 365 days, and no provision was made for the intercalation of an extra day, as we do at leap year. As a consequence the seasons slipped through the year in a way which we may find in the calendar of the ancient Egyptians, which is not so good from the standpoint of the agriculturist, but is especially beneficial to students of calendar, for the time-reckoning remains very clear and unambiguous. The haab was divided into 18 months of 20 days each and a period, perhaps of feasting and

celebration, of five days at the end of the year. In each month (the names of which, too, are known) the days were numbered from 0 to 19 (not, it will be noticed, from 1 to 20; the zero was used by the Mayas in its finest form, as being both a sign of beginning and of completion).

Now we have the complete system of time-reckoning used by the Mayas. Instead of designating a day as being "Friday, July 29, 1 thousand, 9 hundreds, 3 tens, and 2 years A.D.", as we do, they used their own units, corresponding in many ways with ours. A typical Mayan date is

9.16.12.5.17 6 Caban 10 Mol,

in which the sequence of five numbers at the first is the long count, the "6 Caban" is the tzolkin designation, and the "10 Mol" is the designation of the day's appropriate place in the solar year—it is the 97th day of the tzolkin and the eleventh (not the tenth!) day of the eighth month of the haab. In the decimal system this day bears the designation of Mayan Day 1415637, and according to the Spinden correlation between the Mayan and European time counts corresponds to Sept. 2, 503 A.D. (Gregorian).

We may now proceed to the consideration of the origin of the tzolkin. If astronomical phenomena formed the underlying principle, they must have been of such a conspicuous nature that the attention of the Mayas was early drawn to them. If we omit the occasional great comets, the most striking astronomical phenomena are eclipses of the sun and moon, and it is almost positive that if astronomical phenomena do underly the tzolkin we must look for the explanation among the eclipses. But eclipses do not show a periodicity of 260 days, but when we consider the double of this period, 520 days, we do find some sort of periodicity. If we begin with the apparition of a certain eclipse, as set down in Oppolzer's *Canon der Finsternisse*, and go forward 520 days, we come to a date near the actual appearance of a second solar eclipse. Let us use, for example, the eclipse of 736 B.C., Feb. 1 (Julian), as our starting point. Adding 520 days, we come to J.D. 1452785, and a solar eclipse occurs 10 days later. Again progressing 520 days, we come to J.D. 1453305, and an eclipse occurred 7 days earlier. In this way we may count forward

by intervals of 520 days for quite a while without having the difference between the computed date and the real date of a solar eclipse exceed 14¾ days, which is approximately half the synodic period of the moon. In the foregoing example this limit is violated only four times in the first century after the starting point, and three of these vanish if we consider as a starting point not the date of the first eclipse as set down above, but a date about three days earlier.

But by no means are all solar eclipses represented if we merely add 520 days in succession to the date of a real eclipse. Two other sequences are obtained if we count forward by intervals of 520 days from dates respectively 173 and 346 days ahead of the initial date. These three sequences establish all solar eclipses, with but one exception. The solar eclipses which occur in pairs about 30 days apart are not completely treated; only one of such a pair is fixed; but these eclipses are as a rule only partial for the whole earth, and are not both visible in such a limited geographical region such as that occupied by the Mayas. In this way the other member of such a pair probably escaped the attention of the Mayas.

Since for any one place on the earth solar eclipses are rather rare, the Mayas would have had to observe for a tremendously long period of time before being able to recognize these three sequences. But lunar eclipses are much more numerous, and it is a fact that by a suitable choice of an origin both lunar and solar eclipses may be represented by means of the same sequences. All lunar eclipses which appear are predicted, but some of the predictions fail to materialize; there are never more than four eclipses in succession without there being one missing, in a sequence.

Now let us suppose that the Mayas had used 520 days as their "eclipse year" and had given each day of this year a unique designation. They would have seen that eclipses occur on three quite definite days of that year—let us say the Nth, N + 173rd, and the N + 346th days. But the time-interval of 520 days seems to have been too long for them to handle. Perhaps they didn't like it because of its exceeding the solar year and the tun in length; another reason is perhaps that if they were to further subdivide it by means of the vigesimal system, there would be 26 periods of 20 days each, and this would be a further violation of the system in that a factor

exceeding 20 is involved. So they halved it, and chose 260 days for their period. If we assume further that they let the date of the first eclipse fall on the first day of a certain tzolkin, we are sustained by an amazing bit of evidence when we consider the symbol for the first day name, Imix—it resembles a symbol for an eclipsed sun or moon. The Mayas knew the synodic period of the moon, and could count ahead to see the exact date of the new or full moon corresponding to the computed date when they could expect an eclipse of the sun or of the moon. Of course, after a few hundred years, the use of such round numbers meant that the computed and real dates of eclipses drew farther and farther apart, but it is reasonable to assume that in the beginning the tzolkin was started off on just such a principle. That they took no opportunity for an intercalation which would restore the significance of the tzolkin is no more remarkable than that they took no especial care to keep the solar year in harmony with the seasons.

The Spinden correlation between the Mayan Date and our Julian Date is expressed in the equation

$$\text{Julian Date} = \text{Mayan Date} + 489384,$$

and the derivation of this equation is discussed in Spinden's paper *The Reduction of Mayan Dates* (Papers of the Peabody Museum of American Archaeology and Ethnology, Harvard University, Vol. VI, No. 4. Cambridge, Massachusetts, 1924). The true historical beginning of the reckoning is taken as the day

$$7.0.0.0.0 \quad 10 \text{ Ahau} \quad 18 \text{ Zac} = 613 \text{ B.C., August 6 (Greg.)}$$
$$= \text{J.D. } 1497384,$$

as may be verified on page 136 of Spinden's *Handbook*. Employing the eclipses of the years 749 to 614 B.C., taken from Oppolzer's *Canon*, and computing the Julian Dates of the alternate initial days of the tzolkin according to Spinden's correlation, it is found that a solar or lunar eclipse corresponds to every alternate day 1 Imix of the tzolkin; practically all of the differences between the computed and the real dates of eclipses are less than 15 days, with the exception of 6 of 78 solar eclipses and 4 of 73 lunar eclipses, and Ludendorff later shows that these badly discordant eclipses were not observable in the Mayan empire. We see then that the day 1 Imix,

the initial day of the tzolkin, computed on the basis of the Spinden correlation, fulfills very closely the expressed conditions—the solar eclipses occurred on that new moon which was nearest the day 1 Imix, the lunar eclipses at the corresponding full moon. Our assumptions have resulted in a brilliant confirmation of the Spinden correlation.

If we continue the investigation to the point of examining the eclipses which correspond to the use of the other two sequences, respectively 173 and 346 days later than 1 Imix, we find again that all eclipses are satisfied, between the years 749 and 614 B.C., which immediately precede Spinden's date for the historical beginning of the tzolkin reckoning.

If we next examine the error which was made by using the round numbers 520 and 260 days, we find some interesting consequences. The correct values are 519.9275 days and 259.9638 days. The value 520 days is seen to be 0.0725 day too long, and after the passage of a baktun (144000 days) the tzolkin computation gives a residual of 20.077 days, with respect to the true computation; this means that after the passage of one baktun the true eclipses occurred about 20 days earlier than the computed dates. This number, 20 days, or one uinal, must have struck the fancy of the Mayas as being particularly significant. True, it is very significant to us, also, as students of Mayan calendrical design, if we note that after 13 baktuns the discrepancy would amount to one tzolkin of 260 days, neglecting the accumulated additional error of 13×0.077 days, or almost exactly 1 day. And that a great cycle of 13 baktuns played an important part in the thoughts of the Mayas is very well known. The Mayan designation of the beginning of the world was very definitely

13.0.0.0.0 4 Ahau 8 Cumhu,

and after the passage of that baktun the next one was designated

1.0.0.0.0,

and so on, leading up to the value

7.0.0.0.0,

which is believed by Spinden to be the date of the adoption of the calendar in the form in which we have discussed it. Some students of the Mayas are inclined to disregard this idea, because another

violation of the vigesimal system is indicated, but in what has gone above we see a very good reason for such a violation. In fact, it gives a very good reason for the very existence of the number 13 in the calendar of the Mayas. Taking all of the evidence into consideration, it seems extremely likely that the Mayas could have devised the tzolkin as a result of observation of solar and lunar eclipses; when such an assumption is made, the dates of eclipses are remarkably well satisfied by means of Spinden's correlation and Spinden's date of inauguration of tzolkin reckoning, both of which were arrived at on the basis of entirely dissociated material.

ON THE REDUCTION OF MAYAN DATES TO OUR TIME-RECKONING

An attempt will now be made to establish the Spinden correlation on the basis of evidence other than that presented in the first section of this paper. It has been stated that three manuscripts of Mayan origin have survived. One of these, the Dresden Codex, provides the material for this discussion. The upper halves of pages 53 to 58 are associated with the lower halves of pages 51 to 58, and all contain long series of numbers, investigated very completely by Dr. C. E. Guthe (see Vol. VI of the Peabody Museum Papers of Harvard University). The essential content of these tables is a long sequence of the numbers 177, 178, and 148, and these express the numbers of days between eclipses of the sun or the moon; therefore the conclusion must be drawn that in some way the tables of the Dresden Codex present an eclipse calendar. On the upper half of page 52 is the Mayan date

9.16.4.10.8 12 Lamat 1 Muan,

and immediately afterwards, on page 53, the series of numbers begins, so we are forced to accept this date as the initial date of an eclipse calendar. Employing the Spinden correlation, we find that this date corresponds to 496 A.D., January 11 (Julian), and according to Oppolzer's *Canon* the nearest lunar eclipse occurred on 495 A.D., November 18, and the nearest solar eclipse in the same year, on November 3; this latter was only partial, and probably invisible in the Mayan regions. We seem to be faced with the fact that the Spinden correlation fails to confirm our assumptions.

Let us now assume that the Spinden correlation and the suggested time of the tzolkin reckoning, as well as Ludendorff's explanation

of the origin of the tzolkin, are correct. According to the tables of Gr. Guiness (*Creation centered in Christ*; Vol. II. Tables of Vernal Equinoxes and New Moons for 3555 years. London, 1896), the new moon nearest our date above occurred on 496, January 1, and the nearest full moon on 496, January 16. Now the Mayas employed a cyclic reckoning for their lunar phases, and according to Teeple and Spinden (see Spinden's *Handbook*, p. 142) "there is some evidence that the Mayan lunar calendar in the fifth century A.D. had receded about four days from the true positions of the moon". If the date which we have given above as coming from the Dresden Codex represented a cyclic full moon, it is readily seen to correspond with the true full moon which came on January 16th, if we take into consideration this accumulated error of four days and the additional fact that the Mayas counted time only to the even days, and we have no way of knowing whether this was from sunrise to sunrise or from sunset to sunset, or even some other system. A similar accumulation in the error of the tzolkin since its introduction in 7.0.0.0.0 gives as the total error 2.81 (the number of baktuns elapsing) $\times$ 20 days (the error in a single baktun) which results in 56 days. If now we count backwards 56 days from the date given above, we come to the date 495, November 16, only two days before a real eclipse of the moon occurred. The material in the *Canon* enables us to discover that this eclipse began approximately a half hour after the moon crossed the meridian of Copan (one of the important cities of the Mayan Empire) about four degrees north of the zenith. Totality began about an hour later and lasted almost two hours. Even allowing for some uncertainty in the data of the *Canon*, this eclipse must have been an extraordinarily impressive sight in the Mayan Empire.

If now we start with the eclipse of 495 A.D., November 18, and add to this date the numbers 177, 178, and 148 in the order in which they occur in the following pages of the Dresden Codex, we may hope to secure the dates of the succeeding lunar eclipses. Such a tabulation results in differences between computed and true dates of eclipses in the amount of 0 days in 33 cases, and of 1 day in 19 cases; no difference exceeds 1 day, with the exception of one eclipse in the middle of the series which is out by a whole lunation. This

was an exceedingly insignificant eclipse, however, and was probably not noticed at all, if it even occurred (the data of Oppolzer's *Canon* are sometimes in error by appreciable amounts). We might now ask if the Dresden series of numbers will satisfy any other series of eclipses. Of the series of 624 lunar eclipses beginning with the one of 495 A.D., November 18, embracing 400 years, 46 could serve in this way as starting points for the Dresden series of numbers. If we take the ratio of 46 to 624, this is the probability that we could have by chance struck upon such an eclipse for a starting point. Not only is this figure about 1 to 14, but we must also take into consideration that the eclipse which would be thus chosen must have been a conspicuous one, and the one which we have designated certainly fulfills this condition. The total result is that the Spinden correlation with exceedingly great probability may be assumed correct. Of the 53 eclipses thus represented, 33 were easily visible in the Mayan Empire, if we neglect weather conditions, and this is a circumstance which is hardly equalled by any other similar series.

Another question which might now be asked is whether or not the Dresden series of eclipse dates represents observation or mere calculation. It is a bit difficult to answer this question, but if we consider the fact that of those visible to the Mayas 25 differed from the true dates by 0 days, and 7 by 1 day, while of those invisible to them 8 differed by 0 days and 13 by 1 day (including the one which was an entire lunation away), perhaps we may say that actual observations are represented. But then we are faced with the question of why a cyclic date was chosen to start off the series. In all of these considerations we must remember that the mental attitude of a Mayan scholar was very different from that of a modern astronomer, and that therefore many things could have seemed reasonable to him which seem unusual to us. Thanks to the remarkable thoroughness with which the Spanish priests destroyed the Mayan Codices, we know only an infinitesimally small portion of the astronomy of these singularly gifted people. We must consider it a great piece of good luck that the Dresden Codex escaped the pious zeal of such champions of European culture.

Examination of further dates in the Dresden Codex reconciles many of them with cyclically computed dates of eclipses. In addi-

tion, some astronomical phenomena of different nature occurred near dates which are recorded there. For example, a superior conjunction of Venus took place on January 14, 496, 3 days after the date recorded above, and an inferior conjunction of Mercury occurred on January 10, only one day before the date. Another date in the Dresden Codex is 555 A.D., December 27, according to Spinden's correlation, and inferior conjunctions of both Mercury and Venus occurred on December 28; it is an exceedingly rare phenomenon, to have both these planets at the same conjunction on the same day. Whether the Mayas observed these two planets is problematical, but a figure occurs in the Dresden Codex which is remarkably accurate as the synodic period of Venus.

The Astronomical Significance of Pages 51 and 52 of the Dresden Codex

The details of this paper are much too involved to attempt to present them in the space allowed here. It results from Ludendorff's investigations, however, that by combining the numbers and the dates there given, in several ways, dates of true phenomena of Venus as well as several lunar eclipses are represented. An annular solar eclipse is also represented, and it is particularly interesting that by the assumptions made he could have happened upon such a rare event as an annular eclipse of the sun visible in such a limited region as that inhabited by the Mayas. This eclipse was easily visible there, and occurred about 4 p.m., and must have been a very conspicuous phenomenon to the Mayas.

The phenomena such as conjunctions of Venus extend over a period of 239410 days, which was considered by the Mayas to contain 410 synodic periods of the planet. By dividing these two numbers, we find that the Mayas rounded off the value 583.927 days to 584 days, for their calculations; the modern value for the synodic period of Venus is 583.923 days.

The Age of the Moon in the Mayan Inscriptions

In many Mayan inscriptions, in addition to the complete statement of the date as outlined above, there is a so-called "supplementary series" of glyphs which J. E. Teeple has recently succeeded

in deciphering. According to his interpretation, the numerical coefficients of the glyphs known as D and E give the age of the moon on the date defined by the "initial series", as the ordinary group consisting of the statement of the date in terms of the long count, the tzolkin, and the haab is known. That is, the indices of the glyphs D and E indicate the number of days which have passed, at the time expressed by the initial series, since some phase of the moon; Teeple is of the opinion that the phase used was the new moon (perhaps actually the "first light" of the new moon, which follows that phase very closely) and that the date of new moon was always defined by observation. A complete exposition of his results and conclusions are found in a lengthy paper, "Maya Astronomy" (Contributions to American Archaeology, No. 2; Publication No. 403 of the Carnegie Institution of Washington (1930)).

If the Spinden correlation is correct, the dates obtained when the numbers expressed by the glyphs D and E are subtracted from the date of the initial series correspond not at all to dates of new moons, but lie instead about four days before full moons. Quite independently Spinden and Ludendorff came to the conclusion that the glyphs D and E and their coefficients indeed express the age of the moon, but that the lunar phase used for a starting point was not an observed new moon, but a cyclically computed full moon.

Ludendorff chose a series of inscriptions which contain both initial and supplementary series and reduced the former series to Julian Dates and subtracted from them the numbers expressed by the latter series. He then consulted Guiness' table to obtain the dates of the mean new moons (not true), calculated on the basis of a constant synodic period, and added 14.4 days to each date. The mean value of the difference between the two sets of dates was found to be 4.1 days. The differences between this mean deviation and the individual deviations was then taken, and the values found to range between −3.4 days and +3.9 days, a variation of 7.3 days, or about a quarter of a lunation. Even when this large variation is reduced by means of the maximum difference between the true and the mean new moon (0.6 days), and some other small corrections, there still remains an uncertainty of about 5 days, which amounts to the same thing as saying that the Mayas were unable to determine

the time of new moon by observation any more accurately than within 2.5 days, one way or the other. (Ludendorff points out that Teeple is in error when he says on page 47 of his treatise, cited above, that the difference between the true and mean new moons might amount sometimes to 1 or 2 days, on one side or the other.) The values and signs of the differences between the Mayan dates and the dates from the lunar tables indicate that for long periods of time the moon was observed with the same degree of inaccuracy, if we accept Teeple's view, a thing which is highly unlikely.

Teeple based his proofs that the Mayas counted from new moon to new moon on the statements of Bishop Landa, in the first place. Whether any information coming from this source is of much value in such critical discussions is very doubtful, for Bishop Landa never had the confidence of the natives. Second, Teeple states that most primitive people counted the moon's age in this way. This is only partially true, for we find many deviations from this system. Among the Hindoos several different methods have been used—early, the count was from full moon to full moon, and later some of them counted from new to full, and then back again to new, breaking a lunation up into two parts, as was also done by some of the North American Indians. Teeple's next argument is based on the fact that the Mayas counted the age of Venus in this way—from the first appearance of Venus in the rays of the setting sun. But in this case the situation is entirely different, for no other phases of Venus are distinctly marked. Teeple last states that the eclipse calendar of the Dreden Codex must refer to solar eclipses, and hence be dealing with a new moon, but that this is a proof Ludendorff does not admit, for he has shown earlier that the eclipse calendar might be applied with equal accuracy to both lunar and solar eclipses.

If we assume that Spinden's correlation is correct, and that the dates obtained from the combinations of initial and supplementary series correspond to cyclically computed full moon, the difference of 4.1 days corresponds to a lapse of about 860 years since the reckoning was started, for we know that the Mayas (at least at Copan, a very important city) assumed 149 lunations equal to 4400 days. The dates which are discussed by Ludendorff occur about 400 A.D., hence his contention is that about 460 B.C. the cyclic

moon reckoning was established; supported by Spinden's view that the tzolkin and long count reckonings were inaugurated about 600 B.C., this date does not seem at all unreasonable. A closer examination of the dates shows also an accumulation of error which was finally corrected, as though the Mayas realized that the quantity they had been using for the value of the moon's period was inaccurate.

While a number of hypotheses are necessary in order to fully justify the Spinden correlation in this case, it is certain that in no way do Teeple's conclusions contradict the correlation, as seems to be the case at first glance.

The Venus Table in the Dresden Codex

In this last one of the series of five papers of Ludendorff, he deals with a transcription of page 24 of the Dresden Codex, as found in Spinden's "Maya Dates and What They Reveal" (The Museum of the Brooklyn Institute of Arts and Science, Science Bulletin, Vol. IV, No. 1 (1930), p. 89). Except for some undeciphered glyphs, the page seems to contain an array of numbers and dates, from which Ludendorff selects five, on the basis of their location on the page and other considerations too involved to treat here. These dates, when reduced to our calendar by means of the Spinden correlation, run from 350 to 375 A.D., and by means of additions and subtractions of the other numbers on the page from these basic dates, in a very logical order, Ludendorff has reconciled them with the dates of 43 inferior conjunctions of the planet Venus, between 228 B.C. and 583 A.D. The differences between the true and the Mayan dates of the inferior conjunctions are related in a very significant way with the method required to compute the date from the figures given in the Codex. The differences begin large (about −4 days) and decrease to zero, then grow again to about 4 days in the opposite sense, when a new combination of basic dates and auxiliary numbers is used to compute the succeeding dates of inferior conjunctions. Again the differences change until a revision of the method of computation seems necessary, and so on, the actual difference between the true and computed dates never exceeding 5 days. Throughout this period of 811 years the value chosen for the synodic period of Venus remains unchanged at 584 days, the number

2920 days being assumed to be 5 even periods, and appearing over and over again in the Codex. That this is also exactly equal to 8 solar years, or haabs, probably has a great amount of influence in this choice and maintenance of this value for the period of Venus.

Summary

Ludendorff has reconciled many dates in the inscriptions and codices of the Mayas with astronomical phenomena in a rather satisfying way, by the use of the Spinden correlation and numerous original deductions and hypotheses. Growing out of this series of studies is a new interpretation of the origin of the tzolkin period of 260 days, which many noted scholars had pronounced an invention; Ludendorff sets forth proofs that this period was chosen because of its relation to the period intervening between successive lunar or solar eclipses. The overwhelming mass of coincidences with important astronomical phenomena which are brought about by applying the Spinden correlation to the dates of the inscriptions and writings of the Mayas seems to point to the truth of this correlation.

The present writer has checked all of Ludendorff's astronomical data, such as eclipse dates and phases of the moon, and has examined the logic rather closely; the result is that at least we may say that there are no misstatements or inaccuracies in either the facts or the logic. The only adverse criticism which may be offered is that the number of agreements is almost suspiciously large; it seems to be almost too easy to obtain a coincidence with an astronomical phenomenon. However, when we remember that the dates of the inscriptions and writings cover such a tremendous range of time, and are interrupted by periods when there must have been wars, depressions and other evils which beset even us of to-day, we might be reconciled to the rather peculiar distribution of the dates as deduced by means of the Spinden correlation; we must remember, too, that many of the monuments have been destroyed, either by the Spaniards or later inhabitants, or by the elements. Undoubtedly many will yet be found which will provide stops for the gaps which now occur. It is certain that Ludendorff's studies must be considered by the archaeologists.

THE CONSTITUTION OF THE STARS*

By R. K. Young

WHEN we look at the sky on a clear night, we see it covered with a multitude of bright points of light which we call the stars. The early watchers of the skies discovered the fact that the stars keep the same relative positions to each other and called them the fixed stars. They also discovered that there were a small number of objects, to them five, which looked like stars but which did not keep a fixed position and were to be seen at various times of the year at different points in the sky. They called them the planets which means "wanderers."

The nature of the planets was not dicovered till the invention of the telescope by Galileo in the 17th century and the discovery of the law of gravitation by Newton. It was then seen that all the planets were bodies much like the earth, revolved around the sun at distances comparable to the distance of the sun from the earth and shone by reflected sunlight. At that time, only about 300 years ago, the true nature of the stars was not definitely proven. Newton divined that they must be suns shining by their own light but very much farther away than the planets. Newton's reasoning was this—if the stars shine by reflected sunlight then in order to make them appear as bright as they do, one must assume they are very close, or, if far away, correspondingly large, and in either case they would by their gravitational power affect the motions of the planets. Since this was found not to be the case, they must be very remote indeed and not massive enough to exert any appreciable effect on the planets. The only way in order to make them look as bright as they appeared was to assume that they were self luminous. It seemed natural to assume that they might be comparable in brightness to our own sun and, if so, their distance was to be measured in millions of millions of miles. The early deductions of Newton were fully confirmed when the distances of the stars were successfully measured two centuries later. The nearest star we have yet found is some

*Broadcast from Station CFRB in the University of Toronto series of broadcasts, spring term, 1932.

25 million million miles away. If our sun were removed, even to the distance of the nearest star, it would be visible to the naked eye but not very bright. Most of the stars far outshine our own sun. Thus Sirius, the bright bluish star, which rises in the early evening in the south-east now, is some 30 times as bright as our sun and Rigel, the brightest star in Orion, is over 3,000 times as bright as the sun; and indeed most of the stars are brighter, but some are fainter. They have their comparative apparent faintness in virtue of their great distances.

It may seem strange that we can determine the constitution of bodies so extremely remote as the stars. However, if the material cannot actually be obtained within the laboratory for chemical analysis it might almost as well be a million million miles away as one mile. The extreme remoteness is not an insurmountable barrier. All the information comes down to us in the message which their light sends. The feasibility of the problem can be illustrated by two simple analogies. When you listen on the telephone receiver, you are frequently able to recognize the voice at the other end and do not need to be told who is speaking. You recognize the voice, yet no sound is transmitted over the wires. What is transmitted is an electric current, and in this case all the qualities of the voice are impressed on the strength of the electric current. The ear is quite unable to interpret these variations without the aid of the telephone receiver. Radio is another analogy. Messages are flying around the world at all times of the day quite unknown to our senses unless we have the appropriate means of detecting these waves. The stars too are constantly sending down their messages in the form of light waves, but the eye, in many ways a crude instrument, can only distinguish colour and brightness. In order to tell the constitution we must have the right receiving apparatus. This is called the spectroscope, an instrument which analyses the light. Each element sends out its own characteristic light, which we can recognize with the aid of this instrument as definitely as if the substance were in our laboratory. We could, if we were mechanically inclined, make an apparatus so that when pointed to the stars it would actually shout out the words, Hydrogen, Helium, Iron, etc., so definite is our information.

Early workers in the field of spectrum analysis were a little surprised that the elements so common on the earth were also the ones found in the stars, at least with the exception of helium which was first discovered on the sun and only later on the earth. It is now known that the stars are composed of the same elements as compose the earth's crust. This seems to be because of the nature of matter which permits of only a definite number of elements all of which have now been discovered on the earth, and they exist also in the stars.

Possibly more interesting than the researches which tell us the kinds of materials which compose the stars are those which concern the conditions and form in which this matter finds itself. We might divine that stars are very hot because they are self luminous. When we look at them we can see that some are blue, others white, and still others red. Some are as we might say red hot, others white hot, and still others blue. The spectroscope enables us to go further and determine the temperature more exactly. The sun shows a temperature of about 10,000° Fahrenheit. Some of the red stars give temperatures as low as 3,600° Fahrenheit while the blue stars frequently are as hot as 70,000°. These temperatures are so high that all the elements would vaporize and we conclude that the stars are entirely gaseous.

The temperatures we have been talking about refer to the surface, or near surface, of the stars. The interior would be, as you might suppose, very much hotter, but how hot you probably would not correctly guess. It is thought that the deep interior of the sun rises to the fantastic temperature of 30 to 40 millions of degrees. This conclusion is a theoretical one but based on well-established laws. Since the stars are entirely gaseous, and the natural tendency of a gas is to expand, any particle of gas in the interior of a star, must find itself under balanced forces. The gaseous pressure and gravitation, the first of which is definitely related to the temperature. When one considers all the factors and solves the problem we find the high temperatures referred to, and also that the density in the normal stars increases from the very tenuous boundary, which is less dense than our best vacuum, to a density of 30 to 40 at the centre, that is 30 or 40 times as heavy as water and twice as dense

as platinum. This is the normal or average star. All the stars have this in common that the density at their boundaries is zero but the law by which the density increases toward the centre depends on the mass and size of the stars. Some of them are very tenuous and others again very dense. The bright star Betelgeuse, whose diameter has been measured and found to be about 250,000,000 miles, has a volume more than 20 million times that of the sun. Its mass, known from other investigations, however, cannot be more than 100 times that of the sun which means that its mean density is certainly 100,000 times less. This makes it much less dense than air, indeed a very good vacuum. The companion to Sirius on the other hand and one or two other stars while having a mass about equal to the sun are very much smaller and the density of some of these seems to be 50 or 60 thousand times that of water. This density means that a cubic inch of the material composing these stars would weigh, if on the earth, 1 ton.

Another interesting feature of the stars, when we consider their masses and brightness collectively, is that the stars are fairly uniform in mass but differ enormously in their light giving powers. Masses as large as 100 times that of the sun are extremely rare and very few are less than 1/10 that of the sun, while the great bulk have about the sun's mass. But there are stars nearly 100,000 times as bright as the sun and some a million times fainter. At both ends there seems to be a limit. The reasons seem to be that if a star is more than 100 to 200 times as massive as the sun it blows itself to pieces, and if less than about 1/10 that of the sun it rapidly shrinks and ceases to shine.

It is interesting to consider the rate at which many of the stars are pouring out their energy. The sun sends us on each sq. inch of the earth enough heat per minute to raise the temeprature of one gram of water, nearly 3.5°F., and if we work back from this we find that the output of energy at the sun represents 508 thousand billion billion horse power. Even the small fraction of this which strikes the earth represents nearly 5,000,000 horse power per square mile and if the total energy which strikes the earth could be converted into power and sold at the low rate of 1 cent per kilowatt hour its value would be worth nearly 500 million dollars per second. Now many

of the stars are pouring out energy thousands of times faster than this and we ask, where does the heat come from?

If it were simply a question of a hot body cooling down, the stars would not last 10,000 years. We are positive the stars must have been shining for millions of years. Somehow the energy of the stars seems to be replenished. We can explain it if we say that the stars are able to draw on the store of sub-atomic energy. We can explain it also if matter and energy are convertible and that the stars are radiating their mass away.

We find then amongst the stars a remarkable uniformity in many respects but we find an astonishing variation in the temperatures, densities and light giving powers. These conditions cannot be duplicated in the laboratory. It is for this reason that physicists the world over are turning to the stars to answer some of the fundamental questions on the behaviour of matter, and astronomy in the stars provides laboratories which cannot be obtained on the earth.

FURTHER ECLIPSE REPORTS

The following reports were received too late to be included in the last issue.

Editor.

PRELIMINARY REPORT ON METEOROLOGICAL CONDITIONS AT MONTREAL AND LOUISEVILLE DURING AFTERNOON OF AUGUST 31, 1932

By Andrew Thomson

General. From the 8 a.m. weather reports on August 31, it was clear that a mass of air with temperatures between 60 and 65°, was lying over the eclipse belt. This air mass was made up in Northern Quebec largely of air which had lain over the continent for some days while in New England and the Eastern Townships it may have been brought in from the Atlantic. Over Ontario as far west as Port Arthur and over the Mississippi valley, the air was humid and very warm with temperatures up to 80°, which had moved in from the Gulf of Mexico and the Southern States. Over the prairie provinces and Western States lay a great mass of cold air with temperatures down to 50°.

The warm humid air which was lying over Old Ontario, was being pushed up over the wedge of colder air in Quebec. In this process of elevation to greater heights condensation of water vapour took place especially near the interface of cold and warm air, producing strato-cumulus and alto-cumulus clouds with disastrous results to numerous eclipse parties.

Owing to the cloud cover, changes in meteorological elements at the earth's surface were smaller than have been recorded at previous eclipse stations situated far from the ocean.

Barometric Pressure. The eclipse did not produce any measurable effect on the autographic record, and could not have exceeded two hundredths of an inch.

Temperature. The air temperature dropped steadily at all stations from about 2.30 to 3.30, the minimum temperature occurring about 10 minutes after the middle of totality. The following are the differences in temperature readings at 2.30 and 3.30 obtained by different observers at Louiseville: Mr. Carrick, 4.4°F; Mr. Connor 6.6° F; Mr. Hepburn, who was with the London party a mile or two further north, 7.0 °F. At St. Hubert airport near Montreal where the clouds were found by airplane ascent to be about 600 feet thick, the temperature drop was smaller. For the same hour from 2.30 to 3.30, J. F. Carmichael and C. Fontaine at two stations 15 miles apart found a drop of 2.3°F. On Mount St. Hilaire, in the same locality, at a height of 1,300 feet, the temperature fall was 4.3° F. This larger fall at Mount St. Hilaire was doubtless due to the fact that it was above the thickest part of the cloud layer.

Relative Humidity. The relative humidity increased at all stations from about 75% to 95%.

Meteorological Conditions at the Eclipse

Clouds. The clouds at Louiseville were alto-cumulus, and strato-cumulus, moving from the west.

Wind. The wind at Louiseville, which was blowing from 5 to 10 miles an hour from the southwest dropped to a dead calm about 15 minutes before totality, and this calm continued until last contact.

Atmospheric Electricity. The atmospheric electric potential was measured during the period of the eclipse by the Simpson stretched wire method. Two ionium collectors were suspended at a height of one metre by an 18 gauge copper wire 20 metres long. The charge developed on this wire was measured on a Wulf bifilar electrometer at minute intervals. The electrical potential remained fairly constant until 4.12 p.m. (E.S.T.) when there was a sharp decrease to 4.20 and a minimum value was reached 15 minutes after mid-totality. From this time the potential increased slightly until 5.05 when observations ceased.

The decrease of the electrical potential amounted to about 25% at mid-totality, which agrees in general with the results of previous eclipse determinations.

Meteorological Office,
Toronto.

NOTES ON THE VARIATIONS OF TEMPERATURE, HUMIDITY, AND WIND DURING THE ECLIPSE OF THE SUN AT MAGOG, AUGUST 31, 1932: WITH COMMENTS ON THE RISK OF DEW, AND THE USE OF TEMPERATURE OBSERVATIONS

By A. Norman Shaw

Introduction. The observations recorded in these notes were made with a minimum of preparation. A few days before the eclipse, the writer decided to observe variations in temperature, humidity, kata-thermometer readings, and black-body cooling, and gathered the apparatus together hurriedly, with no expectation of making any contribution of importance. Some of the observations are, however, possibly of sufficient interest to put on record, and are submitted below.

The heartiest thanks of the writer are due to Miss M. Dodd, who kindly assisted by taking observations in a most efficient manner, under conditions when rapid readings were difficult to make. The kindness of Colonel F. J. M. Stratton in extending an invitation to come to the Eclipse Camp at Magog was greatly appreciated.

Temperature and Humidity Records. In Fig. 1, records are given of the

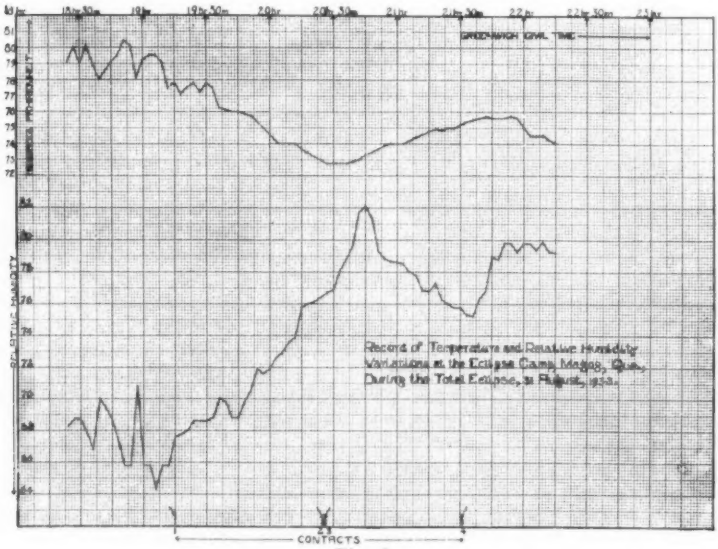

Fig. 1

temperature and relative humidity at the Eclipse Camp at Magog, P.Q., on August 31, 1932, from 1.24 p.m. to 5.15 p.m. (Eastern Standard Time—add 17 h. to give G.C.T.). These were obtained by analysing the records of a calibrated Julien Friez Hygro-Thermograph (No. 2250), and plotting the corrected readings at intervals of three minutes.

In order to make the necessary corrections, and to justify the enlargement of scale the following checks and calibrations were made:—

(1) The temperature recording was checked by comparison with two standardized thermometers at several points on the scale and found, after applying a small constant correction, to be accurate to within 0.1° F when the temperature was not changing rapidly. but, on account of lag, to only within 0.4 °F during the most rapid of the changes which occurred during the period of the eclipse. The differences in temperature at any one time, between various points in the camp (in open shade) were less than two degrees, as observed after the eclipse; this amount of divergence, or greater, is common after any relatively rapid change in the open.

(2) The time was checked by comparison with the official chronograph of the camp, and the corrected times thus obtained are plotted in the figure. A possible variation of 0.5 sec. due to occasional slight sticking of the record pen, is quite negligible on this scale.

(3) The humidity record was checked by comparisons with an Assmann Ventilated hygrometer and standardized thermometer. It was found, after applying a constant correction, to be accurate within 0.2% R.H. in the case of steady readings; but during the changes in humidity as totality was approached and passed, there was considerable instrument lag, estimated roughly to be, in this case, between one and two minutes. The much larger lag seen in the diagram between the maximum of humidity and the minimum of temperature, was, apart from the portion due to this factor, representative of the actual lag in the change of the surrounding humidity. The peak value of the humidity (vertical scale) under changing conditions and reversal of this kind would be expected to vary occasionally at least one or two per cent. from place to place in the vicinity, and hence the accuracy of the maximum recorded value as a characteristic measurement is subject to this limitation.

The Possibility of Deposit of Dew on Apparatus. In regard to the humidity it will be seen from the diagram that the conditions during the approach of the "period of totality" were such that the absolute humidity was nearly constant. For example, assuming that the relative humidity was 69% when the temperature was 78° F (as at about 19 h. 30 m.) a simple calculation shows that, if the absolute humidity remained constant, a temperature of 73° F would lead to a relative humidity of approximately 82%, as was found shortly after the period of totality.

If the temperature had dropped only six more degrees, saturation would have been reached and there would have been a general deposit of dew; indeed some dew must have been formed in cooler places under the existing conditions. Again, if the relative humidity had been 16% higher at the time of the first contact, or

had changed otherwise by the arrival of a sufficiently damp wind, there would have been an equally profuse deposit of dew.

This is apparently a risk of some importance to astronomers, and at any future eclipses observed in humid weather, it should be arranged that the apparatus is protected from cooling. The sudden deposition of dew on mirrors would seriously affect measurements, and constitute a most exasperating disturbance. The writer has not heard of this affecting any past expedition—but probably it has not happened to be sufficiently humid, except on those occasions when the sky was such that the eclipse could not be observed. Occasions will, however, undoubtedly arise when a rift in the clouds on a humid day will give fair conditions for observation accompanied by a sudden deposition of dew sufficiently serious to affect the surfaces of apparatus of small thermal capacity which have not been suitably protected.

The Total Lowering of Temperature. The temperature fell five degrees (Fahr.) between the first contact and just after the third, and then rose three degrees during and after the last phase. As the evening was approaching, the normal cooling at sunset had already commenced, and obviously a complete return was not to be expected even under otherwise uniform conditions. It should be noted, however, that this degree of cooling observed in the open shade occurred under a cloudy sky. It was a transition that represented the effect of passing (on a relatively still day) from conditions under *sun plus cloud* to conditions under *cloud*.

About 1.20 p.m. (E.S.T.) the sun had been unobscured by cloud for over a quarter of an hour and the shade temperature had risen to 84° F. On the quite sudden interception of the sun by clouds very similar to those present during the eclipse the temperature fell finally to 78° F, and then rose with fluctuations as the clouds varied in frequency and density, returning again later to about 78° F as the clouds accumulated and covered the sun almost continuously during the first phase of the eclipse. From this observation it may be deduced that the transition from *unobscured sun* to *sun behind cloud* under the existing conditions was at least six degrees (Fahr.) and probably a few degrees more, because on a still clear day, there would be a further normal rise during the early part of the afternoon.

Hence, on a clear cloudless day of otherwise similar conditions to those of August 31 at Magog, there would have been a total lowering of temperature during the eclipse, of at least the sum of these two differences, namely, eleven degrees (Fahr.). We may confidently estimate, therefore, that a total fall of temperature of between twelve and fifteen degrees would probably have occurred if the sun had not been obscured by cloud. On a dryer day, or with the sun at a higher elevation, there would have been a still greater fall.

A thorough investigation of the fall in temperature at different heights, and of the resultant change in temperature gradient during an eclipse would be of value in dealing with meteorological problems involving the rate of establishment of stability in the atmosphere, and the relative proportions of absorption, transmission and convection of heat near the surface of the earth.

The thermal contraction of metal apparatus becomes appreciable in a fall of the above magnitude and it is obvious that accurate temperature corrections

should be required in all arrangements where thermal contraction or expansion would lead to optical deviation or displacement.

Barometric Pressure. Only approximate readings of pressure were taken, as no appreciable variation was expected. A micro-barograph record might, however, have been of some interest as the fluctuations in the wind were unusually rapid.

A small pocket aneroid (Negretti and Zambra, No. 6286) was used, which gave uncorrected readings (at the altitude of the camp) falling slowly from 29.45 in. at 2 p.m. to 29.40 in. at 4.30 p.m. (E.S.T.). A subsequent comparison of this instrument with two Fortin mercury barometers indicated that without the ordinary barometric corrections, it read 0.15 in. higher than the mercury barometers at 23.0° C.

The Variation of the Wind. The velocity of the wind which varied in direction from between SW. and S., was determined from kata-thermometer readings. Previous comparisons of the kata with a hot-wire anemometer indicated an accuracy of about 5 per cent. in these measurements.

During the period of the eclipse the velocity of the wind fell with irregular fluctuations from 7.0 mi/hr. at 19 h. 27 m. to 0.74 mi/hr. at 20 h. 17 m. During the period of totality it had an average value of 1.7 mi/hr. falling to 0.56 mi/hr.

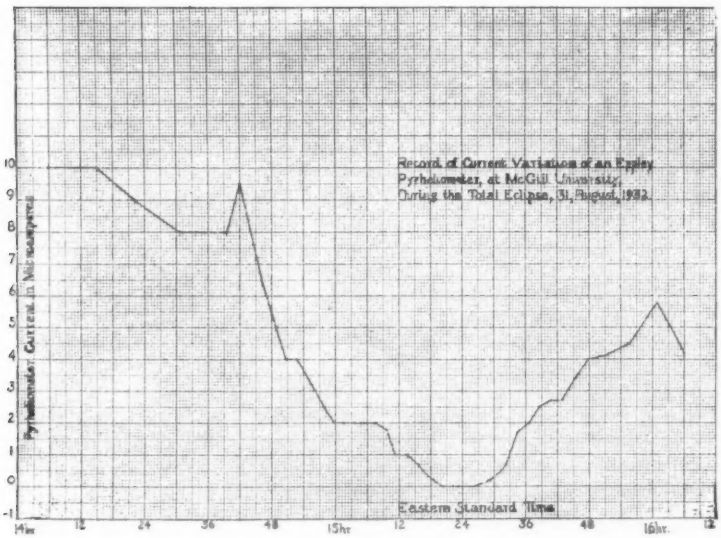

Fig. 2

(By J. Katzman)

at 20 h. 32 m. It rose to 1.26 mi/hr. at 20 h. 45 m. and fell to 0.74 mi/hr. at 21 h. 22 m.

Other Readings. In addition to these readings a continuous record of black body cooling was taken, but owing to a defect discovered subsequently in one of the silvered kata-thermometers it was not possible to perform the desired calculations in which the variation of radiation and wind effects could have been separately estimated. In view of the fluctuating character of the wind this record is, therefore, of little interest, and is omitted.

In Fig. 2 the observations taken by Mr. J. Katzman, with an Eppley pyrheliometer (kindly lent by Mr. M. Eppley) show the variations in radiation, which were occurring during the eclipse under a very similar cloudy sky at McGill University in Montreal. This pyrheliometer (10 junction Weather Bureau Type, No. 295, Res. 30.0 ω) was calibrated by the manufacturers and certified to develop an electromotive force of 1.80 millivolts per gm. cal./min. sq. cm., on the assumption that the relation between radiation intensity and the electromotive force is linear.

Macdonald Physics Laboratory,
McGill University.

REVIEW OF PUBLICATIONS

Das Lokale Sternsystem, von Friedrich Becker, 30 pages.

Die Rotation der Milchstrasse, von H. F. Bottlinger, 63 pages. Berlin: Springer, 1932. Price of complete volume RM. 36.50.

These two separates are from the "Ergebnisse der Naturwissenschaften", volume XI, and contain a summary of the present knowledge of two related subjects. The first paper is chiefly concerned with the stars in the immediate neighbourhood of the sun, while the latter treats of the mechanism of the larger conception, —the milky way. After a critical discussion of the work of Charlier, Shapley, Seares, Pannekoek and others, Becker concludes that the observations are best satisfied by the hypothesis that the *B*-stars form a local group about the sun which is not far from the centre, and the other bright stars are concentric with this group. This group may be a spiral nebula, but it is only one item in a larger galactic system whose plane agrees with the milky way. He regards the evidence for the determination of the extent of the larger system and the distance and direction of its centre as inadequate, the main difficulty being the lack of definite knowledge of the absorption of light in interstellar space.

In the second paper, by Bottlinger, the difficulties of the absorption are not overlooked, but in the major part of the work he reviews researches which are concerned with the motions rather than the brightness.

Starting with the work of Herschel and a Maxwellian distribution of velocities, he traces the historical development of the subject through the discovery of the apex of the sun's way, the vertex of the two-drift hypothesis of Kapteyn, the discovery of the plane of asymmetry by Strömberg and the explanation in terms of the distance effect, and finally shows how Oort's hypothesis of a general rotation of all matter in the galactic plane about a centre of mass under Newtonian dynamical principles explains these various phenomena. The principles of this motion are developed and he finds that the observed phenomena of the various classes of bodies—stars, clusters, nebula, etc.—can be welded together so that

the various bodies form one whole. The various objects revolve about the centre of mass of the galactic system in orbits of various eccentricities and inclinations. There are three systems. System number one comprises the stars of large mass, *W-O-B* stars with part of the *A*'s. This system is the most flattened, that is, the inclinations are around one degree and the eccentricities not over 0.02. The second system comprises stars of intermediate mass, later type stars, planetary nebulae and the majority of the novae. The eccentricities are around 0.1 and the inclinations about three degrees. The third system is composed of the Mira-stars, *RR* Lyrae stars and dwarf *R* and *N* stars. The eccentricities are so high that many members pass close to the nucleus.

The star clusters and magellanic clouds are somewhat outside but revolve around the centre in enormous periods.

R.K.Y.

CHANGE IN THE REDUCTION OF THE OCCULTATIONS OBSERVED IN 1933

By Ernest W. Brown and Dirk Brouwer

Beginning with the first lunation of 1933 (New Moon. Dec. 27, 1932). it is requested that 5″ be subtracted from the moon's mean longitude instead of the 6″ used during the past four years. This is equivalent to the subtraction of .152 $\Delta\alpha$, .152 $\Delta\delta$ where $\Delta\alpha$, $\Delta\delta$ are the variations per minute, or to that of 0^h .00253 from the time at which the moon's position is obtained from its ephemeris. In communicating results, the amount subtracted should be stated.

1932, December 27.

NEWS AND COMMENTS

NOTES FROM THE DOMINION ASTROPHYSICAL OBSERVATORY

The observing statistics will first be given for the past four months in continuation of those in the September issue.

	1932		13-year average	
Month	Nights	Hours	Nights	Hours
August	22	101	24	150
September	27	212	20	145
October	15	101	16	125
November	12	59	12	85

The months of August, October and November were each a little below the average, but on the other hand, this September was the best September since the institution was opened. The fine weather continued over into the middle of October, but since then very few good nights have been secured. The past month holds the record for rain for November for a great many years.

The Director, accompanied by Mrs. Plaskett, went east in August to view the eclipse, and later to attend the meetings of the International Astronomical Union at Cambridge, Mass. He and Dr. Swasey, with whom he was visiting, chose Conway in New Hampshire to view the eclipse, but clouds ruled the day.

Dr. Beals, accompanied by Mrs. Beals, also went east about the same time, going to the eclipse camp at Magog before the Union meetings. After the latter were concluded, they visited their former homes in Nova Scotia for a few weeks before returning to Victoria. As is well known, the camp at Magog, where your scribe was also privileged to be, was unfortunately in the cloudy zone, and while the disappearing crescent of the sun could be seen, no scientific results of value were secured. Those present got a thrill, however, with the oncoming and departure of the shadow.

A number of European scientists had expected to visit Victoria before returning, but to date we have had only Professor W. W.

Heinrich of Prague, who paid us a hurried visit on the 13th of October. He was greatly impressed with the western coast and regretted he had spent so much of his time in the east and middle west that but little of his 30-day trip remained in which to enjoy the Pacific coast.

Another visitor we had was Miss Schmidt, a lady-principal of a high school in Bloemfontein, South Africa, who was on a six month's leave of absence. She was much impressed with the observatory and the work of the local Astronomical Society in the means taken to popularize the science. She plans to try to introduce something similar to our popular talks when she returns to Bloemfontein.

A number of our staff have been on the sick list in recent weeks, but we are glad to report all are back at work again.

W.E.H.

NOTES AND QUERIES

Communications are invited, especially from amateurs. The Editor will try to secure answers to queries.

A Hundred Years Ago.

One often hears remarks about depression and speed and the disheartening outlook for the rising generation, and it may be helpful to learn that conditions were even more discouraging in past years. Recently the French Academy of Medicine celebrated the centenary of one of its distinguished members, Dr. Alexandre Guéniot, who was actually present on the occasion—"an erect, alert figure, smiling attentively at the kindly speeches which one distinguished orator after another delivered". And yet, the day after Alexandre Guéniot was born his father wrote to an intimate friend:—

"I do not know whether to be happy or sorry over the birth of a son to which I have contributed only a modest share. The poor infant enters the world in very troubled times. Hardly seventeen years have passed since peace was restored to Europe, and we still suffer cruelly from the effects of the war. Who knows if my son will not one day be forced to become the citizen of a republic? It makes one shudder. The conditions of life are

daily becoming more difficult. Nanette, our servant, has paid 23 sous for half a kilo of butter, and 2 sous for each fresh egg! It is absurd and exorbitant!

"I should like to see my son embracing the noble career of medicine, but I see quite well that he can not; one of the heads of the faculty has confided to me that this profession is literally invaded. And then, this madness of speed is wearing out men. Only yesterday I saw a post chaise tearing along. It makes one giddy! The horses were galloping at more than 5 leagues an hour. And every one wants his carriage! The streets of Paris are so congested that you must wait a long time if you wish to cross them. Madness of the century, my dear friend, for which men will pay in the brevity of their days.

"My son, like his contemporaries, will not live to be old. We know not what the future has in store for him, but we can bet with certainty on his not becoming a centenarian."

The moral is obvious. We shall pull through our difficulties, and the Editor wishes all the readers of the JOURNAL high courage, good health and success in 1933.

PECULIAR EFFECTS ON ECLIPSE PHOTOGRAPHS.

The present writer's attention has been drawn to peculiar streaks and "ghosts" on some photographs of the recent eclipse of the sun. In some cases there were straight bright streaks through the sun's image, separated equi-angularly. These were on photographs taken just before or after totality. These streaks are undoubtedly due to diffraction at the edges of the small opening in the iris diaphragm of the camera.

In some other photographs there were one or two round enlarged images a little distance from the image of the sun. Among the photographs sent in, this effect was best seen on some taken in Toronto at maximum eclipse (92%) with an ordinary camera, the prints being $2\frac{3}{4}" \times 4\frac{3}{4}$, stop 1/32 and exposure 1/100. The explanation offered is that to produce the first "ghost" the light from the intense image of the sun on the film was reflected back upon the rear convex surface of the lens and from that to the film again, and this may have been repeated to produce the second fainter image.

In some pictures of the partial eclipse, the crescent had a dark central portion. This was, of course, due to solarization produced by over-exposure.

PROGRESS IN ASTRONOMY IN 1932.

Every year-end the *Science News Letter* summarizes the advances made in the different branches of science during the previous year. The following list of achievements in Astronomy in 1932 is given in the issue for December 27.

Eros lost its distinction of being the heavenly body that approaches the earth closer than any other except the moon, as two closer asteroids were discovered, the Reinmuth objeect, discovered by Dr. Karl Reinmuth of Heidelberg, which is the closest, and Amor, discovered by Prof. E. Delporte of the Royal Belgium Observatory.

A total eclipse of the sun occurred on Aug. 31 in New England and Canada while clouds prevented observation by many astronomical parties inland and clear weather prevailed closer to the coast.

The first meeting of the International Astronomical Union to be held in America was held at Cambridge just after the total eclipse.

Radio tests during the eclipse indicated that both portions of the ionized Kennelly-Heaviside radio-reflecting layer of the upper atmosphere are caused by radiations from the sun travelling with the speed of light, not by corpuscles.

The sun may be only half the age now estimated for it if its energy comes from the conversion of hydrogen into heavier elements, Theodore Dunham, Jr., Mt. Wilson Observatory, theorized.

Spectrum photographs of meteors were obtained during the Leonid shower by Harvard College Observatory.

Many comets may be products of eruptions of the surface of Jupiter, Dr. S. Vsessviatsky, of Moscow, theorized.

Ammonia gas was detected in the atmosphere of Jupiter and methane was also detected on the four major planets by Dr. R. Wildt, Göttingen University.

Carbon dioxide is probably present in the atmosphere of Venus, although not previously found on any planet except the earth, Dr. Walter S. Adams, director, and Dr. Theodore Dunham, Mt. Wilson Observatory, reported.

Einstein's prediction of the curvature of light rays near the sun was corroborated by German eclipse photographs studied by Dr. Robert J. Trumpler, Lick Observatory.

Variable stars may vary in brightness because of electrical changes, R. W. Revans, British physicist, suggested.

Flecks of light bordering the Andromeda nebula were provisionally identified as star clusters by Dr. Edwin Hubble of Mt. Wilson Observatory.

The receding motion of distant nebulae does not decrease the velocity of the light which travels from them to the earth, Dr. Gustaf Strömberg, Mt. Wilson Observatory, found; his work was confirmed independently by Dr. George Van Biesbroeck, Yerkes Observatory.

Seven new comets and five returning periodic comets were sighted during the year.

The bicentennial of the birth of America's first astronomer, David Rittenhouse, was celebrated.

An age of the universe of not much more than 3,000 million years was estimated by Dr. Ernst J. Opik, Esthonian astronomer working at Harvard.

A new fireproof astrophotographic building at Harvard was opened to house 400,000 glass astronomical negatives.

Progress was made on the mirror and mechanical parts for the 40-inch telescope of the U.S. Naval Observatory.

An 80-inch reflecting telescope was planned for McDonald Observatory, Texas.

A 61-inch reflecting telescope, fourth largest in the world, was planned for the new Oak Ridge station of Harvard College Observatory.

A new apparent astronomical speed record was observed at the Mt. Wilson Observatory when two nebulae in the Gemini cluster were found to have a spectral red shift that indicates they are rushing away from earth at nearly 15,000 miles (24,000 km.) per second.

The Leonid and Perseid meteor showers were widely observed but the Leonids were not as plentiful as astronomers had hoped on the strength of a 33-year cycle based on the great showers of 1833 and 1866.

Clouds of gas or dust cause reddening of light passing through space and for this reason stars and clusters near the plane of the Milky Way are only a quarter to a half the distance previously estimated, it was found by Dr. Joel Stebbins, working with a photo-electric cell attached to the Mt. Wilson 100-inch telescope.

Old Astronomical Records in Japan.

The active and enthusiastic amateur astronomer, Mr. Yasuaki Iba, of Ohte, Kobe, Japan, has just published "Fragmentary Notes on Astronomy in Japan", a 34-page booklet in English, excellently printed. In it Mr. Iba quotes many astronomical references in old literature, such as the following:

In the 8th moon of the 6th year of Emperor Jomei, *i.e.*, 634 divided into 12 hours, beginning at midnight.

During the 8th moon, 15th year of Suinin Tenno, the 11th Emperor, *i.e.*, 15 B.C., the people ". . . witnessed stars falling like a shower and were very miserable indeed".

In the 8th moon of the 6th year of Emperor Jamei, *i.e.*, 634 A.D., there was "observed a long star in the south and it was called a comet".

On the 8th of the 9th moon of 764 A.D. "a huge meteorite, as big as a waterpot, fell upon the roof of the bedchamber of Oshikatsu Yemi".

The pamphlet has many such records and Mr. Iba is to be commended for his publication.

Was this an Auroral Display?

Mr. Bert Huffman of Langdon, Alberta, sends the following account of a striking celestial exhibition which was seen at 6 p.m., M.T., December 15, 1932.

"It emerged from the horizon in a narrow column and spread out fan-wise as it arose. Its direction was from a little south of east to the northwest. As it advanced up and across the sky it thinned out perceptibly and broke into small floating clouds, moving as the Northern Lights often shift. Toward the horizon it remained a rather heavy mist-like light tinged just the slightest with pink, but in its upper areas it thinned to the lightest filmy mist. The entire duration until it had entirely died away was only about fifteen minutes. The sky was clear, the moon not yet above the horizon and all the early evening stars were as shown in my crude drawing. I hope you receive other reports of this beautiful exhibition."

Death of Weston Weatherbee.

The editor has just learned of the death at Albion, N.Y., of Weston Weatherbee, a long-time member of the R.A.S.C. He was formerly sheriff of Orleans County, N.Y., and well known as an amateur astronomer. In 1904 he presented to our Society a reflecting telescope containing a Brashear mirror 8½ inches in diameter.

C.A.C.

MEETINGS OF THE SOCIETY

At Winnipeg

October 12, 1932.—The opening meeting of the winter session was held in the University Arts Building at 8.15 p.m.

The President, Dr. Neil John Maclean, addressed a few words of welcome to the assembly, after which he referred to the visit of the members of the Royal Astronomical Society, England, to Winnipeg in August last.

Mr. W. H. Darracott, 773 McMillan Ave. was elected to membership in the Society.

The speakers of the evening were Prof. V. W. Jackson, Mr. D. R. P. Coats and Dr. L. A. H. Warren. These members had gone east to view the eclipse of the sun in August, and each gave an account of his impressions.

Prof. Jackson saw the eclipse from Sorel. His remarks were illustrated by means of splendid lantern slides. Mr. Coats gave a vivid description of the eclipse as seen from an airplane, from which he was broadcasting events as he saw them, thousands of feet above cloud-girt Montreal. Dr. Warren, stationed less than one mile from Magog, where events proved disappointing to the British astronomers, had an unusual bit of good luck, when the rift in the dense clouds opened for a few moments and stayed open until totality came, and thus gave to him and his party an opportunity of seeing the eclipse in all its grandeur.

A social half-hour followed, during which time the ladies of the society, under the convenorship of Mrs. J. H. Kolb, served refreshments. Mrs. Neil John Maclean and Mrs. E. L. Taylor presided at the tea table.

S. C. Morris, *Secretary.*

At Vancouver

October 18, 1932.—The ninth regular meeting of the Vancouver Centre, the first for the 1932-33 session was held at the University of British Columbia. Dr. Shrum was in the chair.

In answer to requests for the installation of a question or suggestion box, Mr. Teasdale was asked to take charge of this.

Dr. Shrum then introduced the spaker of the evening, Mr. Brand of the Department of Mathematics of the University of British Columbia, whose subject was "Einstein and Astronomy". Mr. Brand began by declaring his intention of discussing the approach of the mathematician to the laws of nature. The theory of relativity is to be considered as an imaginative and in a certain sense a poetic structure. In the geometry of Euclid, certain definitions and axioms are established, from which the theorems may then be derived. One doubtful postulate made by Euclid was that through a given point there can be drawn one,

and only one, line parallel to a given line. This postulate does not appear to be self-evident, and admits of no proof.

Bolyai and Lobatchevsky attempted to justify this postulate by assuming it not true and developing theorems which they expected to lead to contradictions, but no such contradictions were found. A two-dimensional world based on the ideas of Lobatchevsky has been discussed by Poincaré. It takes the form of a circle with the temperature varying in such a way that it becomes zero at the circumference. In such a world the shortest distance between two points is no longer a straight line but the arc of a circle and a large number of lines may be drawn through a given point parallel to a given line.

A third treatment was given by Riemann in 1854 when he considered the case of no parallel line being possible, this applying to a world of the nature of the surface of a sphere.

The speaker then proceeded to consider the application of such ideas to the physical world. Fundamentally we have to deal with the distance between two points, positions being located with reference to certain coordinate axes. Riemann's postulate gives a different expression for that distance than that of Euclid.

The famous "ether-drift" experiment of Michelson and Morley was then described, and attention drawn to the negative result obtained. Fitzgerald and Lorentz were able to explain this result by assuming the contraction of a moving body in the direction of its motion. Einstein pointed out that this contraction need only be apparent, and was connected with the method of transmitting signals and the variation in the times for the two systems.

The necessity for a difference in the times as measured by observers in systems in motion with respect to one another becomes apparent when we consider Maxwell's equations. If the velocity of light is to be the same for all systems it is not sufficient that a transformation of space coordinates be made. A corresponding transformation for time is involved, and this introduces the conception of the interdependence of time and space.

Einstein's principles then involve the constancy of the velocity of light, the impossibility of detecting anything but relative motion, and the idea of a space time continuum. These were put forward in his paper of 1905 as applied to uniform motion only. In 1916, however, he considered non-uniform or accelerated motion. In this case space is considered Riemannian in character, and the treatment leads to a new theory of gravitation.

On the experimental side, relativity theory has been able to account for the magnitude of the deflection of light in passing the sun; the advance of the perihelion of Mercury, and the shift of spectral lines in a massive body.

Einstein's present work consists in an attempt to obtain a new kind of mathematics which will serve to identify gravitation and electromagnetics. In 1929 the unitary field theory was a partial solution of this problem.

The speaker concluded by drawing attention to the conception of a universe finite yet unbounded.

WILLIAM URE, *Recorder.*

The Royal Astronomical Society of Canada

OFFICERS FOR 1932

Honorary President—R. M. STEWART, M.A., F.R.S.C., Ottawa.
President—R. K. YOUNG, Ph.D., Toronto.
First Vice-President—MGR. C. P. CHOQUETTE, M.A., Lic.Scs., Montreal.
Second Vice-President—LACHLAN GILCHRIST, M.A., Ph.D., Toronto.
General Secretary—R. A. GRAY, B.A., 198 College St., Toronto.
General Treasurer—J. H. HORNING, M.A., 198 College St., Toronto.
Recorder—E. J. A. KENNEDY, Toronto. *Librarian*—R. A. GRAY, B.A., Toronto
Curator—ROBERT S. DUNCAN, Toronto.
Council—D. S. AINSLIE, M.A., Ph.D., Toronto, Ont.; NAPIER DENISON, Victoria, B.C.; MISS A. VIBERT DOUGLAS, Ph.D., Montreal; R. E. DELURY, M.A., Ph.D., Ottawa; E. A. HODGSON, M.A., Ottawa; A. R. HASSARD, K.C., Toronto; J. A. PEARCE, M.A., Victoria; J. PATTERSON, M.A., Toronto; JOHN SATTERLY, M.A., D.Sc., Toronto; L. A. H. WARREN, M.A., Ph.D., Winnipeg; and Past Presidents—SIR FREDERIC STUPART; C. A. CHANT, M.A., Ph.D.; A. T. DELURY, M.A.; L. B. STEWART, D.T.S.; J. S. PLASKETT, D.Sc., F.R.S.; A. F. MILLER; J. R. COLLINS; W. E. W. JACKSON, M.A.; R. MELDRUM STEWART, M.A.; A. F. HUNTER, M.A.; W. E. Harper, M.A.; H. R. KINGSTON, M.A., Ph.D., and the Presiding Officer of each Centre as follows—J. S. Lane, B.A., Ottawa; DR. L. V. KING, Montreal; N. J. MACLEAN, M.D., Winnipeg; P. H. HUGHES, Victoria, B.C.; E. H. MCKONE, London, Ont.; W. S. MALLORY, M.A., Hamilton; G. M. SHRUM, Ph.D., Vancouver.

TORONTO CENTRE

Honorary President—SIR FREDERIC STUPART. *Chairman*—R. A. GRAY, B.A.
Vice-Chairman—A. R. HASSARD, B.C.L. *Secretary*—E. J. A. KENNEDY
Treasurer—DR. W. M. WUNDER *Recorder*—S. C. BROWN
Curator—R. S. DUNCAN *Asst.-Curator*—F. L. TROYER
Observation Committee—DR. C. A. CHANT, J. R. COLLINS, A. R. HASSARD, A. F. HUNTER, DR. D. B. MARSH.

OTTAWA CENTRE

Hon. President—C. C. SMITH, B.A., D.L.S. *President*—J. S. LANE, B.A.
Vice-President—R. GLENN MADILL
Secretary—MISS M. S. BURLAND, B.A., Dominion Observatory, Ottawa
Treasurer—A. W. GRANT, B.A.
Council—A. H. MILLER; J. P. HENDERSON; N. A. IRWIN, B.A.; and Past Presidents—R. M. STEWART, M.A.; R. E. DELURY, M.A., Ph.D.; R. J. MCDIARMID, M.Sc., Ph.D.; C. R. COUTLEE, C.E.

MONTREAL CENTRE

Honorary President—MGR. C. P. CHOQUETTE *President*—DR. L. V. KING
First Vice-President—G. HARPER HALL, *Second Vice-President*—DR. JULIAN C. SMITH
Secretary-Treasurer—DR. A. VIBERT DOUGLAS, McGill University
Council—DR. A. S. EVE; LT.-COL. W. E. LYMAN; JUSTICE E. E. HOWARD; H. E. S. ASBURY; DR. BELL DAWSON; MISS THIBAUDEAU; DR. F. R. ENGLAND; DR. C. C. BIRCHARD; DR. A. H. MACCORDICK; PROF. N. B. MCLEAN.

LONDON CENTRE

Hon. President—H. R. KINGSTON, M.A., Ph.D., F.R.A.S.
President—E. H. MCKONE, B.Paed. *Vice-President*—T. C. BENSON
Secretary-Treasurer—G. R. MAGEE, B.A., S.M., 427 William St.
Council—F. H. COATES, A. CALLARD, F. W. DALY, F. FISHER, G. A. MINHINNICK.

WINNIPEG CENTRE

President—N. J. MACLEAN, M.D. *Vice-President*—A. W. MEGGET
Treasurer—J. H. KOLB *Secretary*—MRS. J. NORRIS, 569 Sherburn St.
Council—D. R. P. COATS; RT. REV. T. W. MORTON; MRS. E. L. TAYLOR; J. HOUSTON; PROF. V. W. JACKSON; DR. L. A. H. WARREN; MRS. H. M. SWEET; A. V. THOMAS.

VICTORIA CENTRE

Honorary President—J. A. PEARCE, M.A., Ph.D. *President*—P. H. HUGHES
First Vice-President—N. C. STEWART *Second Vice-President*—CAPT. J. N. HUGHES
Secretary-Treasurer—H. BOYD BRYDON, 2390 Oak Bay Avenue, Victoria, B.C.
Council—J. P. HIBBEN; JOHN DEAN; REV. J. GOODFELLOW; CHAS. HARTLEY; DR. FRANK HOGG.

HAMILTON CENTRE

Hon. President—REV. DR. D. B. MARSH, Norwich, Ont.; *President*—W. S. MALLORY, M.A.; *Vice-Presidents*—REV. JOHN SAMUEL, T. H. WINGHAM, PROF. W. FINDLAY, Ph.D.
Secretary-Treasurer—GEO. E. CAMPBELL, 142 Gladstone Avenue.
Curator—HUGH RODGER; *Council*—DR. GORDON M. JACKSON, MISS W. PARKER, W. T. GODDARD, DR. W. R. JAFFRAY, W. G. MILNE, W. HODGSON, J. A. LITTLE, S. C. SCAMMELL, MISS A. B. MARSH.

VANCOUVER CENTRE

Hon. President—DEAN D. BUCHANAN. *President*—G. M. SHRUM, M.A., Ph.D
Vice-President—STANLEY BURKE. *Recorder*—H. GRAYSON SMITH, M.A., Ph.D.
Treasurer—J. L. BENNETT.
Council—MRS. M. A. KELK, MRS. C. M. ROBSON, F. R. WILLIAMS, T. J. MCGILL, J. A. MACKENZIE, H. A. PITMAN, A. OUTRAM.

EDMONTON CENTRE

President—DR. J. W. CAMPBELL *Vice-President*—PROF. E. S. KEEPING
Secretary—MISS FRANCES FISHER, 10953-123 St. *Treasurer*—DR. E. H. GOWAN
Councillors—FATHER ENGLEBERT, MRS. J. G. TAYLOR, MISS M. SIMPSON, CAPT. H. J TOWERTON, MR. K. F. ALEXANDER.

A GENERAL INDEX

to Volumes 1-25 of the Journal of the Royal Astronomical Society of Canada and to its previous publications (dating from 1890) is now ready.

The Index has been prepared by W. E. Harper, M.A., Assistant Director of the Dominion Astrophysical Observatory, Victoria, B.C. Subjects and names are arranged in a single alphabetical list, with numerous cross references.

About 125 pages, price $1.00 (postage, 5 cents extra). Remittances may be sent to J. H. Horning, General Treasurer, 198 College St., Toronto.